

International Organizations Count

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This special issue seeks to move forward the development of an empirical research agenda that takes seriously the complexity of how international organizations (IOs) function and the need to study that complexity at all levels of analysis by using robust research tools. We advocate for a broad empirical research approach that molds and sharpens theories about IOs by conducting systematic tests in large-sample environments. Two themes create a common thread throughout this issue. First, shifting the focus from whether IOs matter to how they work requires acknowledgment of the contingency of cause and effect. A second common thread lies in the authors' treatment of IO membership as an aggregate phenomenon—that is, as a set of institutions and relationships evolving over time and with many members rather than as a single organization.

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The growing need to manage international cooperation has led to a steady rise in the number and prominence of international organizations (IOs) and in the use of formal international agreements more generally.¹ This trend has been accompanied by contrasting speculation, either that treaties and international organizations are capable of achieving something like international amity or that they

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reflect little more than the usual practice of state power. Nearly a decade ago, scholars sounded a call to arms. Traditional debates about whether or not IOs “matter” were excessively dogmatic. Earlier controversies involving categorical claims eclipsed more nuanced and productive questions about variation in institutional design and discouraged attempts to identify and gauge observable effects (Martin and Simmons 1998). The question of *whether* IOs matter was to be replaced by an exploration of *how* they operate in world affairs.

The discipline has begun to meet this challenge, as scholars from various backgrounds focus attention on IOs and the mechanisms of international cooperation. Still, despite innovative research by a growing number of scholars, the empirical study of international cooperation has not kept pace with theoretical developments (Frieden and Martin 2002; Goldstein, Rivers, and Tomz 2007). This special issue seeks to move forward the development of an empirical research agenda that takes seriously the complexity of how IOs function and the need to study that complexity at all levels of analysis by using robust research tools that make better inference possible.

Our approach in this special issue is different from most: we do not advocate a particular paradigm of international cooperation, dependent or independent variable, or unit of analysis for study. Indeed, the articles in this issue employ a variety of theoretical perspectives and explore a range of questions and approaches to analysis. We are not concerned with promoting a given theory or inciting more research on one specific variable. Rather, we are advocating for a broad empirical research approach that molds and sharpens theories about IOs by conducting systematic tests in large sample environments. The articles unite in their desire to understand better the complex ways in which different kinds of IOs function and in their commitment to empirical tests relying on large samples. We view the endeavor as cumulative, partial and complementary to other research approaches. The insights derived from quantitative research illuminate, especially in the aggregate, how and why IOs are created, function, and evolve. Statistics are a tool that, in conjunction with other research methods, helps scholars unearth and solve important international cooperation puzzles and identify important sources of variation. The articles showcased here should thus be viewed as a step in the process of building a better understanding of IOs and of international cooperation more generally.

Challenges in International Organizations Research

Current IO research faces several challenges. Although we have rich theories of what IOs do and how they work, until recently, much of our evidence was biased or unsystematic. Theoretical debates were often addressed through anecdotes or relied solely on the use of case studies. While useful for uncovering possible causal mechanisms and for developing and communicating intuitions about process, even carefully chosen case studies pose problems for generalization that are well known (Slantchev, Alexandrova, and Gartzke 2005). Quantitative methods are now widely

accessible and are used by many IO scholars. In our view, scholars should more actively use these tools in cautious and creative ways to move the discipline forward by developing critical tests that prune available explanations and offer further refinements of theory.

Consider research on the relationship between IOs and conflict, an area rich in theory and ideas. For years, scholars made the case variously that IOs promote peace or exacerbate political violence (Angell 1913; Domke 1988; Haas 1958; Laski 1933; Mitrany 1933; Zimmern 1936). Over time, authors articulated different causal mechanisms through which this relationship might operate. Some argued that IOs prevent conflict by legitimating collective decisions and changing perceptions of identity and self-interest (Deutsch 1957; Finnemore 1996; Johnston 2001; Oneal et al. 1996; Russett, Oneal, and Davis 1998). Others emphasized IOs' role in facilitating reciprocity (Axelrod and Keohane 1985) or enabling states to fashion credible commitments (Fortna 2003, 2004). Still others argued that IOs prevent conflict by making disputes costly, by establishing conflict resolution mechanisms, or by changing state preferences from conflict to peace (Gartzke, Li, and Boehmer 2001; Mansfield and Pevehouse 2000; Pevehouse and Russett 2006; Russett and Oneal 2001; Stone Sweet and Burnell 1998).

In contrast, critics charged that IOs have no real influence in world affairs (Jervis 1982; Mearsheimer 1994/95; Schweller 2001). Indeed, as a venue for power politics, the functioning of international organizations has been described as largely "epiphenomenal" (Mearsheimer 1994/95). While all of the above explanations are interesting, and most can be considered plausible under certain circumstances, it is very unlikely that they are all equally correct. Testing has so far been difficult. Despite the richness in thinking about how IOs shape the politics of war and conflict, until recently, researchers who did engage in more sophisticated analyses were limited by meager IO data (cf. Oneal and Russett 1999; Russett and Oneal 2001; Russett, Oneal, and Davis 1998; Shanks, Jacobson, and Kaplan 1996). Scholars have only recently begun to unpack the evidence systematically, cross-nationally, over time, and/or across different institutions or institutional features (cf. Boehmer, Gartzke, and Nordstrom 2004; Doyle and Sambanis 2006; Leeds 2003a, 2003b; Leeds, Long, and Mitchell 2000; Pevehouse and Russett 2006).

This problem extends far beyond the study of IOs and militarized conflict; more evidence is needed in all kinds of areas where theories are abundant but evidence and critical testing still lag behind. In economic affairs, scholars often highlight the role of IOs as commitment devices, arguing, for instance, that IOs enable democratic leaders to credibly signal their trade liberalization preferences (Mansfield, Milner, and Rosendorff 2002), promote cooperation on economic sanctions by establishing issue linkages (Martin 1993), and discourage states from imposing illegal trade protection by making the threat to "see you in court" more credible (Allee 2005). Simmons (2000) and Simmons and Hopkins (2005) argue that signature of Article 8 of the International Monetary Fund Treaty constrains state behavior by

focusing the expectations of individuals, firms, and other states and by increasing the reputational costs of noncompliance. Von Stein (2005), in contrast, maintains that Article 8 serves primarily as a screening device, exerting little constraining power independent of the factors that lead states to sign. These insights are important. Now, we need to know much more about whether and how these processes play out.

Theories are also plentiful in environmental affairs. Young and Levy (1998) argue that IOs enhance cooperation by promoting norm internalization, which ultimately leads states to view the related agreements as legitimate.² Others maintain that the design of the agreement itself is what matters most for (non)compliance: environmental treaties that heighten the transparency of compliance and reduce implementation costs are most likely to succeed (Mitchell 1994). For Keohane, Haas, and Levy (1993), environmental IOs provide an outlet for the expression of domestic political demands for protection. Others are more skeptical, suggesting that chosen pollution targets by and large reflect actions that states are already taking (Murdoch and Sandler 1997). Still others charge that because environmental IOs rarely possess strong inspection programs or sanctions for noncompliance, there is good reason to be skeptical of their overall effectiveness (Ringquist and Kostadinova 2005). Which ideas have leverage, and under what conditions? Systematic, cross-national evidence is still hard to come by (Mitchell 2002). As in the realm of security studies and IOs, institutional research on the environment needs more systematic testing using large samples.

In the realm of human rights, some argue that international treaties and organizations are reasonably effective tools for protection: They constrain national sovereignty and curtail repression; they serve as justification for action and as venues that shape interests and beliefs about appropriate behavior; they activate transnational legal processes which, over time, set in motion improvements in human rights practices; and they signal governments' preferences for reform (Chayes and Chayes 1993; Franck 1988; Koh 1999; Lutz and Sikkink 2000; Simmons n.d.). Others contend that human rights treaties are, by and large, not effective in achieving compliance, particularly among those states most guilty of abuses or most in need of reform (Hathaway 2002; Hafner-Burton 2005; Neumayer 2005; Hafner-Burton and Tsutsui 2005). Although the body of sound empirical research on international human rights organizations and institutions is growing, many features of these IOs have not yet been measured, and therefore it has not been possible to pit arguments against each other in a systematic fashion. Debates about whether and how these institutions work remain unresolved or come down to case-by-case debate.

In the face of deep and diverse convictions about how IOs operate, theories of international organization have developed much more quickly than has reliable empirical evidence (Goldstein, Rivers, and Tomz 2007). While quantitative research on IOs existed as early as the 1960s (cf. Haas 1961; Jacobson 1967; Russett 1966; Weigart and Riggs 1969),³ these studies focused almost exclusively on the UN and

its subsidiary bodies. As a result, the statistical literature provided a fairly narrow snapshot of the universe of international cooperation. Beginning in the 1970s, a few scholars began to examine a broader range of IOs, particularly in the conflict literature (cf. Jacobson, Reisinger, and Mathers 1986; Wallace and Singer 1970). Although these studies were an important step forward, the coding of the IO variables was fairly rudimentary, the methodological tools available to researchers were often insufficient to test the theories of interest, and the lack of familiarity with statistical methods among international relations scholars led to problems in disseminating the findings of early research.

Statistical Analyses: Accelerating the Growth of Knowledge about IOs

Today, quantitative studies of IOs are more common and more accepted by the wider academic community, and scholars are increasingly applying statistical methods in their search for better understanding. Still, this research has not yet generated the critical mass of scholars, literature, and feedback that have proven so productive in other fields. A number of studies have already begun this process;⁴ we hope to pick up the pace and make improvements. Of course, multiple research methods are both necessary and productive in the goal of understanding what institutions do and how they are created, maintained, or defeated. The compelling case for quantitative analysis is not in the numbers, per se, but in the ability of this research method to yield findings that scholars can in turn apply to hypothesis testing and theoretical refinement. In our view, IO research will best serve the effort to provide cumulative knowledge when particular examples are placed in the context of larger samples. While successes or failures of particular cases, such as the UN, are often due to idiosyncratic factors, larger samples that include information about many countries and years allow us to better assess which factors reliably predict the outcome(s) of interest and under what conditions they do so.

Contingency in Cause and Effect

Two themes create a common thread throughout this issue. First, shifting the focus from whether IOs matter to how they work requires acknowledgement of the *contingency* of cause and effect. Nations sometimes cooperate despite IOs and fail to cooperate even in the presence of them. IOs exhibit a wealth of institutional designs, causes, and effects. At times they clearly promote cooperation, but at other times they seem to exacerbate conflict, produce perverse outcomes, and evolve or act in ways that states did not anticipate or do not want. The complexity of IOs mandates research methodologies that are able to tackle “noisy,” endogenous relationships and embrace the diverse ways in which IOs, so rich with variation, affect

world politics. Contingency demands research methodologies that are capable of capturing tendencies, moving beyond rough claims about whether IOs matter to a domain of variability and degree.⁵ Because data collection on IOs is hard and the mechanisms of international cooperation are complex, few studies deal with contingency empirically. This is starting to change, thanks in large part to the efforts of scholars conducting large-sample research.

The articles in this issue uncover a number of interesting contingencies in cause and effect. Dorussen and Ward argue that IOs' primary contribution to interstate stability does not necessarily lie in their immediate impact but rather in the indirect linkages resulting from an accumulation of IOs. Hafner-Burton and Montgomery maintain that the same preferential trade agreements that under some circumstances help smooth over problems between members that can lead to war or human rights abuses do not prevent economic sanctions. Moreover, they find, linkages resulting from an accumulation of these institutions may even provoke sanctions. For Mansfield and Pevehouse, states' decision to join IOs is contingent upon domestic regime type as well as the type of international institution on offer. Von Stein emphasizes the contingent effect of agreement design and compliance costs on ratification: whereas relatively "soft" law often garners relatively widespread participation in international environmental agreements, "harder" commitments may deter from joining the very states whose practices are least consistent with the treaty's requirements. For Hansen et al., global IOs' success in conflict management results chiefly from their high institutionalization and use of arbitration and adjudication. Regional IOs' success, in contrast, is largely attributable to their homogenous and democratic nature.

IO Membership as an Aggregate Phenomenon

A second common thread in this issue lies in the authors' treatment of IO membership as an aggregate phenomenon—that is, as a set of institutions and relationships evolving over time and with many members rather than as a single organization. This approach enables us to draw more generalizable conclusions than is possible when examining evidence from individual organizations. Moreover, it allows the analyst to differentiate between institutional characteristics inside the same type of IO or across different IOs. But there are many different ways to think about, and implement, aggregation. For Mansfield and Pevehouse, aggregating IO membership and distinguishing between types of institutions provides insight into when and why governments choose to enter particular organizations. Hansen et al. are interested in how different institutional characteristics encompassed in states' overall IO membership portfolio affect conflict behavior. The critical point here is that focusing on any single organization is unlikely to tell us the whole story: when thinking about how IOs affect state behavior, it is important to understand the entire portfolio of IOs to which a state belongs or a representative sample of it.

Dorussen and Ward, Hafner-Burton and Montgomery, and von Stein all employ some form of social network analysis, a particular kind of aggregate approach that has generated considerable interest in other fields but has attracted relatively little attention in the international relations literature.⁶ Network analysis is useful not only because it provides insight into how states' overall IO membership portfolio (rather than their participation in one regime alone) shapes behavior but also because it considers how the connections generated by membership affect the mechanisms of cooperation. Dorussen and Ward show that the IO network provides direct and indirect communication channels between states, where indirect links can act as partial substitutes for direct diplomatic ties. Hafner-Burton and Montgomery demonstrate that social networks of IOs, in their case, preferential trade agreements, create relative disparities in power between states that shape whether or not economic sanctions are used. Von Stein examines the conditions under which social networks generated by states' overall IO membership affect state ratification of the core treaties of the international climate change regime. All three articles bring a structural perspective to the study of IOs that would be hard to identify through case studies alone.

Differences and Disagreements

The articles in this issue explore a range of questions and employ a variety of theoretical approaches. Why do states create and/or join IOs (Mansfield and Pevehouse)? How do institutional features, and/or the networks they create, affect state behavior (Dorussen and Ward, Hafner-Burton and Montgomery, Hansen et al., von Stein)? Some of the articles cover specific issue areas. Others examine important overlaps across issue areas. We view these differences and even disagreements between the articles as an important part of the knowledge-building process. Where the contributors disagree over analytic approach, we hope to spur debate about how best to model the complex processes of international cooperation. Where the contributors have differing theoretical expectations, dialogue and comparison of empirical results can help generate additional theoretical refinement and testing. Rather than cultivating "paradigm wars," however, differences of the type exhibited in this issue are amenable to the process of debate, evidence, cumulation, and eventually, consensus.

One fruitful area of disagreement between the articles in this issue is the costs states incur in joining IOs. Mansfield and Pevehouse assume that IOs are generally quite costly to join. From this perspective, IO participation should increase the costs of defection. Moreover, these authors maintain that institutional selection is driven primarily by domestic politics, as states often join to express credible commitments to reform. This contrasts with other views articulated in this issue. Von Stein agrees that domestic politics affects institutional selection in the

environmental arena but maintains that the costs of being a party depend critically on agreement design and the extent to which state behavior coincides with what the treaty prescribes or proscribes. With respect to trade agreements, Hafner-Burton and Montgomery argue that IO networks create differences in their costliness to members and that these differences in turn generate variable defection problems or differences in information diffusion.

The articles in this issue also diverge occasionally in their conclusions about whether and when the proliferation of IOs is good news for international conflict and cooperation. Dorussen and Ward and Hansen et al. contend that IOs are, in many circumstances, successful conflict managers (either directly or via the linkages they create). Mansfield and Pevehouse suggest that IOs can solve commitment problems, particularly for democratic and democratizing states looking to implement difficult reforms. For von Stein, whether the proliferation of international environmental agreements is good news for cooperation is decidedly mixed: although certain flexibility provisions can help, the evidence also indicates that states avoid making deep commitments when these involve “hard” legal commitments.⁷ The same is true for Hafner-Burton and Montgomery: preferential trade agreements have different effects on different types of conflict behaviors, preventing some (such as war) but perhaps exacerbating others (such as sanctions) in certain situations.

The contributions to this issue also differ in their approaches to analysis. Dorussen and Ward, Hafner-Burton and Montgomery, and Hansen et al. look to IO design traits and membership to explain outcomes. Mansfield and Pevehouse and von Stein look to these very design traits to explain IO membership. Finally, the articles sometimes reach different conclusions about the role of power. Mansfield and Pevehouse find that great power states are quite likely to enter into IOs. Hafner-Burton and Montgomery, however, emphasize that IOs have the potential to become sites of struggle over power.

Moving Ahead

A growing body of large-sample research on IOs is materializing; this issue represents one step in that broader process. By taking seriously IO contingency in cause and effect, the contributors offer new insight into the rich and complex world of IOs. By considering IOs as an aggregate phenomenon, and subjecting them to scrutiny in the context of large samples, the articles provide more generalizable conclusions and more astute differentiations of institutional characteristics than would otherwise be possible. They also paint a more colorful and accurate portrait of how IOs are designed, selected, and modified and how IO memberships affect various outcomes. The differences and even disagreements between the authors provide a forum for debate over theory, method,

and data that we hope will spill over into the community of IO scholarship more palpably.

The articles in this special issue also raise flags about the limitations of statistics as a tool of social inquiry. As has long been noted, although we gain generalizability when using statistics, we also lose the ability to explain details of specific cases and to convey deep intuitions about process. This is particularly problematic if, as a result, we overlook factors—such as culture or legitimacy, for instance—that are important but are difficult to measure and/or compare across cases. We view statistics as a complement to—but not a substitute for—in-depth case studies. What is more, scholars relying primarily on statistical methods should be in constant dialogue with researchers and practitioners who have extensive on-the-ground knowledge and experience. Another significant challenge (perhaps not particular to statistics) lies in the difficulty of gauging the consequences of IOs. If IO creation and membership are at least in part endogenous to states' expectations about subsequent cooperation (Downs, Rocke, and Barsoom 1996), what is the best way to model these relationships statistically? Although this question has generated some debate in recent years (Simmons and Hopkins 2005; von Stein 2005), it has not reached a consensus.⁸

As quantitative research on IOs continues, we hope that scholars will tackle the numerous questions that remain. One significant area of ambiguity involves comparisons across types of cooperative behavior. We have included in this issue studies that analyze treaties and intergovernmental organizations as well as the networks they create. We have set aside the question of how and why states choose between formal and informal mechanisms, an important area for future consideration. Are formal institutions a sign of cooperation, or do they indicate a failure to cooperate through informal mechanisms? What are the benefits and drawbacks of formal institutions versus informal norms? Part of the objective here is to begin to think of IOs as a set of mechanisms and behaviors that sometimes complement each other and at other times compete. Extending this approach to less formal institutions will provide further insight into the mechanisms of international cooperation.

A great many other questions deserve attention. What impact does the increasing number of IOs have on the formation of new organizations? We know little, in fact, about the dynamic effect of IOs, though much speculation exists that deserves additional testing and theoretical refinement. Moreover, the diffusion of IOs, often uneven in different regions, should have effects best represented by network analysis (a subject of considerable attention here). In this respect, we hope that this special issue will lead to further theorization and empirical testing about how IO and other interstate linkages affect international relations. Finally, nongovernmental organizations have become increasingly active on the international scene in recent years, and yet we know little about how they interact with IOs and with states. When do nongovernmental organizations (NGOs) “go international” and when are they effective? When do IOs cooperate actively with NGOs, and when do they

impede their participation? Is the rise in NGO activity good or bad news for international cooperation?

We hope that the present set of studies helps to accelerate the process of research and testing by spurring a community dialogue about what we know, what we do not yet know, and where we go from here. Nevertheless, knowledge is cumulative and must always be subject to further questioning and refinement. Further exploration of these questions will in turn lead to challenges or refinements of existing theoretical claims. We emphasize *process* as opposed to *outcome*: we wish to advance large-sample scientific inquiry but do not mean to imply that we are far enough along to offer a body of systematized knowledge. In our view, it is more important for readers to become engaged in the process of inquiry than to embrace (or reject) the findings of particular articles in this issue. Further inquiry and debate are precisely what we hope to prompt. We welcome this process as critical in advancing the state of knowledge about international cooperation.

Notes

1. Throughout, we employ the vernacular of the subfield, using the term *international organization* (IO) to describe the broad body of formal international agreements and organizations.
2. Finnemore (1996) makes a similar argument with regard to the UN Educational, Scientific, and Cultural Organization.
3. See Alger (1970) for an overview.
4. In addition to the studies discussed here, these include, among others, Boehmer, Gartzke, and Nordstrom (2004); Busch and Reinhardt (2003); Doyle and Sambanis (2000, 2006); Goldstein, Rivers, and Tomz (2007); Hafner-Burton (2005); Hathaway (2002); Ingram, Robinson, and Busch (2005); Jensen (2004); Kelley (2007); Koremenos (2005); Leeds (2003a, 2003b); Leeds, Long, and Mitchell (2000); Leeds and Savun (2006); Mansfield and Bronson (1997); Mansfield and Pevehouse (2000); Mansfield and Reinhardt (2003); Mattes (2006); Morrow (2007); O Neal and Russett (1999); Pevehouse (2002a, 2002b); Przeworski and Vreeland (2002); Rose (2004); Russett and O Neal (2001); Simmons (2000, n.d.); Simmons and Hopkins (2005); von Stein (2005, 2007); Vreeland (2003); Walter (1997); Ward (2006); and Werner and Yuen (2005).
5. A few existing studies investigate IO contingency systematically (cf. Fortna 2003, 2004; Goldstein, Rivers, and Tomz 2007; Hafner-Burton 2005; Leeds 2003a, 2003b; Leeds and Savun 2006; Mattes 2006; Morrow 2007; Simmons n.d.; Walter 1997).
6. Hafner-Burton and Montgomery (2006); Ingram, Robinson, and Busch (2005); and Ward (2006) are exceptions.
7. We follow Downs, Rocke, and Barsoom's (1996) definition of *depth*: the extent to which the treaty requires states to depart from what they would have done in the absence of a treaty.
8. For an innovative approach to this problem, see Tomz (2007).

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